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Peer Review File

Seasonal sea-ice in the Arctic’s last ice area during the Early Holocene

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18th Oct 22

Dear Dr Detlef,

Your manuscript titled "Seasonal sea ice in the Arctic's last ice area during the Early Holocene" has now been seen by 3 reviewers, and I include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and to the editorial threshold as outlined below) to assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. In particular, we ask that you:
• Provide an in-depth, balanced discussion, including more existing paleo-oceanographic data, particular the ostracods and provide robust support for your conclusions.
• Provide a detailed evaluation of the used biomarker approach
• Provide new insights about the global average surface temperature rise and Arctic Amplification

Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Ilka Peeken, PhD
Editorial Board Member
Communications Earth & Environment
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Joe Aslin, PhD
Senior Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMATTING

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All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate

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DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

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Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

The paper "Seasonal sea ice in the Arctic’s last ice area during the Early Holocene" has the excellent aim to investigate sea ice fluctuations in the Lincoln Sea north of Greenland and Canada during a time interval ca. 13,000 years ago when global temperatures were naturally warm. The Lincoln Sea is a key area to investigate as it holds the thickest sea ice in the Arctic Ocean and is part of stabilising sea ice in the region today. The paper presents novel data on sympagic and phytoplankton biomarkers in addition to TOC (total organic carbon) from two marine sediment cores in addition to fifteen core tops (multicores). Seven and five radiocarbon dates from the two cores cover the intervals ca. 4,900 – 9,700 and ca. 2,900-10,100 14C years respectively. IP25 (sympagic biomarker) and brassicasterol (phytoplankton biomarker) occur together showing that the core sites have been occasionally ice free during the early Holocene. This is also supported by the TOC values. These results are compared to mean annual air temperatures based on ice cores from the neighbouring Agassiz Ice Cap on Ellesmere Island and further put into a modern context. These results are of interest to the biomarker community, but the paper in its current state is not of interest to a wider field for several reasons.

Since the study by Müller et al. (2009) the field on HBIs have developed, and it is less robust only to include IP25, HBI II and brassicasterol in biomarker studies. Generally, additional data from other paleoceanographic proxies would make the paper more robust.

Modern sea ice decrease is mainly due to atmospheric forcing, and when discussing mechanisms behind the sea ice dynamics in the Lincoln Sea during early Holocene ocean temperatures are not included (e.g., Polyakov et al. 2020; Cronin et al., 2022). The observation of an ostracod species by Cronin et al. (2022) is included in the discussion as the species points to the same sea ice conditions as the current data. The same study also includes benthic foraminifera including a species indicating relatively warm Atlantic water which the current study would benefit from taking into account.

References

Müller, J., Masse, G., Stein, R. & Belt, S. T., 2009. Variability of sea-ice conditions in the Fram Strait over the past 30,000 years. Nature Geoscience 2, 772-776, doi:10.1038/ngeo665

Polyakov, I. V. et al., 2020. Borealization of the Arctic Ocean in Response to Anomalous Advection From Sub-Arctic Seas. 7, doi:10.3389/fmars.2020.00491

Cronin, T. M. et al. , 2022. Holocene paleoceanography and glacial history of Lincoln Sea, Ryder Glacier, Northern Greenland, based on foraminifera and ostracodes. Marine Micropaleontology 175, 102158, doi:10.1016/j.marmicro.2022.102158

Reviewer #2 (Remarks to the Author):

Overall Comments

The Last Ice Area (LIA), situated to the north of the Canadian Arctic Archipelago and Greenland, contains the world's oldest and thickest sea ice. It is also forecast to be the last region to lose its perennial ice cover. As a result, the LIA will be an important refuge to ice dependent ecosystems in the coming decades. It is however unclear how stable the LIA and thus some uncertainty in its ability to act as a refuge. This is a very interesting paper that reconstructs sea ice variability within the LIA during the early Holocene, a period characterized by warm conditions similar to what we will be experiencing over the next few decades. The authors find a transition to seasonal ice cover occurred during this period of warmth. They conclude that even if we keep the global temperature increase since pre-industrial times below 2oC, a transition to seasonal ice cover in the LIA is 'imminent.' This is a very interesting and important paper that will be of wide interest and warrants publication. My expertise is on the current state of sea ice in the LIA and it is from this perspective that I reached my conclusions. I do have some issues that I am of the opinion need to be addressed before publication.

Major Comments

1) Focus on sea ice concentration

The unique aspect of the LIA is the presence of old and thick sea ice. There are other regions of the Arctic where, at least during the winter, there is seasonal ice cover with concentrations that approach 100%. For example, Figure 1 places the work in context and shows the trend in annual sea ice concentration. The LIA appears as a region where the trend is close to zero. Had they shown the trend in annual sea ice thickness (for example as shown in Moore et al (2021;'Spatiotemporal Variability of Sea Ice in the Arctic's Last Ice Area'), a much more dramatic picture of the LIA's precarious state would be provided. The LIA is losing thick ice at an accelerated rate compared to the rest of the Arctic. Granted, sea ice thickness is a more challenging field to monitor as compared to sea ice concentration and the observed records are shorter. The authors do use results from PIOMAS which is an observationally constrained Arctic sea ice and ocean model and so I would strongly recommend that they use this ice thickness data set in the paper. This includes both Figure 1 as well as Figure 3 and Supplementary Figure 6.

2) Relationship with global average surface temperature rise

In several places in the paper (Abstract and Lines 206-207), reference is made to a rise in global mean surface temperature. This is of course relative to some baseline and the authors should be more careful in how they refer to this.

3)Arctic Amplification

The Arctic is currently warming at an accelerated rate compared to the rest of the globe. This is a well known observational result and not, as suggested by the authors (Lines 215-216) a prediction of future changes. I suggest that the authors look at Serreze and Berry (2011: 'Processes and impacts of Arctic amplification: A research synthesis') for a review of the observational evidence for amplified warming at high northern latitudes. The wording of the sentences around this issue is a bit convoluted (for example, it is never explicitly stated that the warming is restricted to high northern latitudes). The reference to the amplified warming at higher elevations is most likely limited to Tibet. I doubt the models have sufficient resolution to resolve the Agassiz Ice Cap.

Kent Moore

Reviewer #3 (Remarks to the Author):

This is an exciting study that illuminates a system for which we have extremely limited proxy evidence: namely, past sea ice conditions in the Arctic Ocean. In particular, this study presents a Holocene sea ice reconstruction from north of Greenland, a region expected to host some of the last perennial Arctic sea ice as the world warms in the future. It is vital to understand how sea ice conditions in this "last ice area" responded to past warming, and the Holocene Thermal Maximum is the most recent reasonable analog for future conditions. This study presents primarily biomarker-based evidence for loss of perennial sea ice in the Lincoln Sea during what was likely the warmest part of the early Holocene. Ostracode data from a prior study apparently reinforce this paper's key conclusions.

I find this to be timely, novel, important, convincing and generally clearly presented work, and I recommend it for publication in Communications if the authors can clarify some points and add additional discussion. Some topics (as described below) warrant more thorough overview of the existing literature.

Major comments:

I think the authors have done a good job with a somewhat challenging set of 14C ages. (And a challenging set of ages is what one would expect from the Arctic Ocean!) The actual uncertainty of the age model probably exceeds what is implied by the uncertainties specified for ages of events in the core, but I'd argue it can't be well quantified and that the detailed, thorough info provided in Methods allows readers to decide for themselves how much confidence to place in modeled ages. The authors may want to consider reducing the precision of the ages they assign to key events in the core, ie rounding to 1000 yrs and/or qualifying with "approximately" or something similar, to reflect more realistic precision of the age model.

Somewhere (perhaps ~ line 67), the paper should clearly articulate the relationship between sea ice conditions in the central Arctic Ocean and in the LIA/Lincoln Sea. Is the LIA the presumed last refuge of land-adjacent sea ice in the AO, or projected to be the very last bit of sea ice anywhere in the AO? Ie Is it presumed that if/when the LIA lost perennial sea ice in the past, the central AO must have been without perennial sea ice too? Or not? Either way, build the case with reference to model simulations and first principles. This will make the implications of this study's findings much more clear later in the paper. Ie does seasonal sea ice in the Lincoln Sea during the Holocene Thermal Maximum mean there was NO perennial sea ice *anywhere* in the Arctic Ocean? (Or rather, that there was probably no summer sea ice anywhere *near land* around the Arctic basin? Extremely important distinction from a climate perspective.)

Also clarify the point above (relationship with central Arctic Ocean sea ice conditions) in the abstract and first paragraph of the text.

Line 92/Fig 2: The mention of an ostracod species that likely indicates open water conditions during the early Holocene inferred "low ice period" is a great complement to the biomarker data, and significantly strengthens the key conclusion of this paper. But the ostracode angle seems just sort of "thrown in" to the paper and I want more info. How well does occurrence of Polycope align with the biomarker evidence, for example? It appears the data are presented in a separate 2022 paper, but can you show the published ostracod species (assemblage?) data here in Fig 2? At least, show occurrence vs absence of the key ostracod taxon Polycope.

I'd like to see acknowledgement and a brief discussion of the "binary" nature of the signal here in biomarkers. In particular, is it surprising that the early Holocene period of seasonal sea ice would end so abruptly in an (essentially permanent, except for one "blip" in the mid Holocene and the recent change) transition to perennial sea ice? The sharpness of that change in biomarker abundance makes me wonder if something other than a climate shift is responsible, ie if something drove a fundamental change in overall sedmentation processes at the site at that time. But did any other basic sedimentological parameters change at the same time? It's hard to tell when comparing Supp Figs with Fig 2 by eyeballing.

Lines 121-130: Two issues I'd like to see discussed here. One, what role is diagenesis likely to play in composition of these surface sediments vs deeper sediments? And two, is it likely that the last century dominates signals in these surface sediments given the stated durations they likely represent? WAsn't the past 1 ka until the 20th Century especially cold in the Arctic, and if so, why are values so high? Overall, what I'm getting at is: How comparable are these surface values to downcore values (given diagenesis) and what time period do they likely represent?

Lines 132-135: In addition to the Agassiz ice core data, there are additional archives and proxies that speak to early Holocene atmospheric temperatures and sea ice conditions in this region that should also be part of this paper's discussion. This would strengthen the paper's case for the link between summer air temperatures in the region and sea ice conditions in the Lincoln Sea, and avoid the appearance of cherry-picking one record for comparison. Even if Agassiz is the only proxy record with which direct quantitative comparisons are made, the broader literature should be discussed and acknowledged. This study should be grounded in the broader literature from the region. Axford et al 2021 Annual Review of Earth and Planetary Sciences reviews temperature evidence from Greenland. Or as examples of individual studies, this paper could make interesting links to the ice cap histories reconstructed in North Greenland by Larsen et al 2019 Boreas (which proposes that reduced early Holocene sea ice and associated higher precip may have helped some ice caps in northern Greenland survive the thermal maximum), and summarize more sea ice reconstructions eg from Georgiadis et al 2020 Marine Geology on Holocene sea ice in Nares Strait.

Lines 206-218: I really appreciate the effort to link a paleoclimate record directly to implications for the future. However, there's a problem with this argument, in its assumptions about seasonality. Above you argued for the link between summer air temperatures and sea ice conditions – but here you discuss mean annual air temperatures. You cannot assume that the modern relationship between JJA and MAAT held up in the early Holocene. Indeed, seasonality was most likely very different in the early Holocene given insolation forcing. Reconsider your rationale here, with careful attention to non-stationary seasonality through the Holocene.

Additional comments:

Abstract line 21: As mentioned above, it's not only Agassiz data that show warm summer air temperatures in this region in the early Holocene. Broaden scope.

Abstract line 22: suggest replacing "amplified temperatures" with "amplified warming" or "amplified temperature change"

Fig 1 caption: Cool figure. For the sake of your broad readership, clarify at this first use what is meant by annual sea ice concentration trend in % per decade. % of what exactly?

Line 52: so long-term stability/prognosis is unknown. Set this up by summarizing the range of reasonable climate model projections/results. Do models disagree or is there a consensus projection? Current wording here implies no consensus ie model disagreement, but describe the state of knowledge more precisely.

Line 60: See my previous requests regarding presentation of age modeling.

64: Reference 19 is not a great one to make this point here, since it has very limited Arctic data. Choose a reference that includes (or even emphasizes) Arctic data eg Kaufman's 2004 Arctic HTM synthesis in QSR, or Kaufman et al 2020 Scientific Data for a global perspective that includes the Arctic.

Fig 2: What do the authors make of the increase in IP25 in the late Holocene in both cores (especially notable in core -14)?

143-144: By "numerical studies" do you mean climate models? This seems unconventional terminology. And more importantly, is this a consensus view or do models vary? Either way is interesting – just clarify.

150-152: This point abou the Lomonsov Ridge data is important, but hard to make sense of as the paper is currently written. More clearly articulating the relationshpi between future & past ice conditions in the central AO vs the LIA (earlier in the paper, per my earlier comment) will hopefully help.

154: This is a key conclusion of your paper, but it needs to be clarified with respect to conditions in the central AO, per my earlier comments.

159: again, "numerical investigations" = numerical climate model simuations?

182-183: Maybe I am missing something obvious, but I don't understand the temperature values given here. Are these meant to be anomalies relative to a baseline, and if so, why eg is HTM 5 degrees cooler (?) than pre-ind? I think there's something in the wording here that is confusing. Clarify or correct.

Fig 3 caption (or elsewhere): clarify what the Agassiz temperature inferences are based upon (ie what are the proxies – melt and d18O)

198: decreased (not decrease – fix tense)

Supp Info: Some of the main points in the "Additional climate mechanisms" section of Supp Info should, in my opinion, be at least summarized briefly in the main text of the paper, to provide a more nuanced discussion within the paper itself. There is a nice, rich discussion within Supp Info that is helpful and paints a necessary, much more complete picture of possible influences on early Holocene sea ice in the LIA/Lincoln Sea. Authors could integrate key aspects of it into the main text to significantly strengthen the discussion.

Review of COMMSENV-22-0811-T

Reviewer #1

Thank you very much for your comments on our manuscript 'Seasonal sea ice in the Arctic's last ice area during the Early Holocene'. We appreciate the time and effort you have put into improving our work and provide detailed answers to your comments below:

1. Since the study by Müller et al. (2009) the field on HBIs have developed, and it is less robust only to include IP25, HBI II and brassicasterol in biomarker studies. Generally, additional data from other paleoceanographic proxies would make the paper more robust.

   Thank you for this comment. With regard to similar comments from reviewer #2 and #3 we have included a more detailed comparison of biomarker and benthic foraminiferal and ostracod assemblages in Ryder19-12GC1 and Ryder19-14-GC1 (line 115-140, Fig. S6, Fig. S7). These support our findings based on the biomarker records.

2. Modern sea ice decrease is mainly due to atmospheric forcing, and when discussing mechanisms behind the sea ice dynamics in the Lincoln Sea during early Holocene ocean temperatures are not included (e.g., Polyakov et al. 2020; Cronin et al., 2022). The observation of an ostracod species by Cronin et al. (2022) is included in the discussion as the species points to the same sea ice conditions as the current data. The same study also includes benthic foraminifera including a species indicating relatively warm Atlantic water which the current study would benefit from taking into account.

   As stated in the answer to the previous comment, we have included records of the dominant benthic foraminiferal species as well as important ostracod species (lines 115-140, Fig. S6, Fig. S7). This includes *C. neoteretis* and *Polycope* spp., which both have been associated with waters of Atlantic origin (Cronin et al., 1994; Poirier et al., 2012; Seidenkrantz, 1995). Additionally, in the supplementary material (lines 32-51) we discuss a potential role of Atlantic Water in the observed sea ice signal. As the reviewer states, atmospheric forcing (temperatures and circulation) likely plays a dominant role for sea ice in the Arctic Ocean, thus additional mechanisms are discussed in the supplementary material.

References

Cronin, T.M., Holtz, T.R., Whatley, R.C., 1994. Quaternary paleoceanography of the deep Arctic Ocean based on quantitative analysis of Ostracoda. Mar. Geol. 119, 305–332. https://doi.org/10.1016/0025-3227(94)90188-0

Poirier, R.K., Cronin, T.M., Briggs, W.M., Lockwood, R., 2012. Central Arctic paleoceanography for the last 50 kyr based on ostracode faunal assemblages. Mar. Micropaleontol. 88–89, 65–76. https://doi.org/10.1016/J.MARMICRO.2012.03.004

Seidenkrantz, M.-S., 1995. Cassidulina teretis Tappan and Cassidulina neoteretis new species (Foraminifera): stratigraphic markers for deep sea and outer shelf areas. J. Micropalaeontology 14, 145–157. https://doi.org/10.1144/JM.14.2.145

Review of COMMSENV-22-0811-T

Reviewer #2

Dear Kent Moore,

Thank you very much for your time and effort to improve our work on sea ice reconstructions in southern Lincoln Sea across the Holocene. We very much appreciate your 'more modern' sea ice perspective and are happy to hear that you find our work to be interesting and important. We provide detailed responses to your comments below.

Kind regards,

Henrieka Detlef and co-authors

1. Focus on sea ice concentration
   The unique aspect of the LIA is the presence of old and thick sea ice. There are other regions of the Arctic where, at least during the winter, there is seasonal ice cover with concentrations that approach 100%. For example, Figure 1 places the work in context and shows the trend in annual sea ice concentration. The LIA appears as a region where the trend is close to zero. Had they shown the trend in annual sea ice thickness (for example as shown in Moore et al (2021;'Spatiotemporal Variability of Sea Ice in the Arctic's Last Ice Area'), a much more dramatic picture of the LIA's precarious state would be provided. The LIA is losing thick ice at an accelerated rate compared to the rest of the Arctic. Granted, sea ice thickness is a more challenging field to monitor as compared to sea ice concentration and the observed records are shorter. The authors do use results from PIOMAS which is an observationally constrained Arctic sea ice and ocean model and so I would strongly recommend that they use this ice thickness data set in the paper. This includes both Figure 1 as well as Figure 3 and Supplementary Figure 6.

   Thank you very much for this comment. We agree that sea ice thickness in the LIA is potentially more diagnostic of the ongoing change compared to sea ice concentration. However, we deliberately chose to show sea ice concentration, as this variable is more comparable with sea ice proxies for past sea ice conditions (Belt and Müller, 2013). Dramatic changes in ice thickness can also be considered a precursor for eventual loss of sea ice concentration, and so with concentration we are looking at a more extreme situation in the last ice area. Thus, a comparison of sea ice *concentration* and atmospheric temperatures in the modern may be more representative of the reconstructed Early Holocene dynamics than sea ice thickness.

2. Relationship with global average surface temperature rise
   In several places in the paper (Abstract and Lines 206-207), reference is made to a rise in global mean surface temperature. This is of course relative to some baseline and the authors should be more careful in how they refer to this.

   Thank you very much for making us aware of this issue. We have revised the manuscript accordingly and added the respective baseline (e.g. compared to preindustrial; 1850-1900) to all mentions of specific temperatures.

3. Arctic Amplification
   The Arctic is currently warming at an accelerated rate compared to the rest of the globe. This is

a well known observational result and not, as suggested by the authors (Lines 215-216) a prediction of future changes. I suggest that the authors look at Serreze and Berry (2011: ‘Processes and impacts of Arctic amplification: A research synthesis’) for a review of the observational evidence for amplified warming at high northern latitudes. The wording of the sentences around this issue is a bit convoluted (for example, it is never explicitly stated that the warming is restricted to high northern latitudes). The reference to the amplified warming at higher elevations is most likely limited to Tibet. I doubt the models have sufficient resolution to resolve the Agassiz Ice Cap.

Thank you very much for this comment. It was not our intention to make it sound that Arctic Amplification is only a feature of future warming. As such, we recognize that the wording in this section was ambiguous and decided to delete it. With regard to your comments and comments from reviewer #3, we have revised the section ‘Implications for future sea ice dynamics in the Lincoln Sea’ (lines 240-317). We draw on additional records to highlight the complexity of reconstructing Early Holocene Arctic temperatures (lines 290-302) and suggest that mean annual atmospheric temperatures in the Lincoln Sea region may have been similar to what is expected for a 2 °C global warming scenario with regards to Arctic Amplification.

References

Belt, S.T., Müller, J., 2013. The Arctic sea ice biomarker IP_{25}: a review of current understanding, recommendations for future research and applications in palaeo sea ice reconstructions. Quat. Sci. Rev. 79, 9–25. https://doi.org/10.1016/j.quascirev.2012.12.001

Review of COMMSENV-22-0811-T

Reviewer #3

Thank you very much for your detailed review and evaluation of our manuscript. We appreciate your work and thank you for your very constructive and helpful comments. We have provided detailed responses to all comments below.

1. I think the authors have done a good job with a somewhat challenging set of 14C ages. (And a challenging set of ages is what one would expect from the Arctic Ocean!) The actual uncertainty of the age model probably exceeds what is implied by the uncertainties specified for ages of events in the core, but I'd argue it can't be well quantified and that the detailed, thorough info provided in Methods allows readers to decide for themselves how much confidence to place in modeled ages. The authors may want to consider reducing the precision of the ages they assign to key events in the core, ie rounding to 1000 yrs and/or qualifying with "approximately" or something similar, to reflect more realistic precision of the age model.

   Thank you very much for this comment. Indeed the ^{14}C ages presented in the manuscript are not straightforward. Thus, we have adapted your suggestion and added statements of uncertainty (approximately, circa, ~) to the ages presented for key events. We do not agree that rounding the ages to the nearest 1000 or 500 would provide a better age estimate than our stated ages with added uncertainties of ±600 years. Rounding without stating uncertainties might give the false impression of better age control than provided by the current model. Therefore, all our reported ages remain rounded to 100 years and we now include a large uncertainty of ±600 years to acknowledge these challenges in the core chronologies.

2. Somewhere (perhaps ~ line 67), the paper should clearly articulate the relationship between sea ice conditions in the central Arctic Ocean and in the LIA/Lincoln Sea. Is the LIA the presumed last refuge of land-adjacent sea ice in the AO, or projected to be the very last bit of sea ice anywhere in the AO? Ie Is it presumed that if/when the LIA lost perennial sea ice in the past, the central AO must have been without perennial sea ice too? Or not? Either way, build the case with reference to model simulations and first principles. This will make the implications of this study's findings much more clear later in the paper. Ie does seasonal sea ice in the Lincoln Sea during the Holocene Thermal Maximum mean there was NO perennial sea ice *anywhere* in the Arctic Ocean? (Or rather, that there was probably no summer sea ice anywhere *near land* around the Arctic basin? Extremely important distinction from a climate perspective.)

   Thank you very much for your comment. This is a very important question regarding the relationship of sea ice in the LIA and the Arctic Ocean as a whole. Climate model projections for the 21st century generally agree that the coastlines of northern Greenland and Ellesmere Island facing the Arctic Ocean will be the last refuge of summer sea ice in the Arctic Ocean (lines 55-59). Thus, as suggested by the reviewer, by first principle, a transition to seasonal sea ice in the LIA would be equivalent to a shift to seasonal sea ice in the entire Arctic Ocean.

   On the other hand, the sites analyzed as part of this manuscript are near-coastal sites. Thus, we cannot rule out completely that the observed signal is related to a breakdown of the perennial landfast ice cover in the LIA associated with formation of leads along the coast of northern

Greenland. Nonetheless, the breakdown of the landfast ice would be related to thinning and increased mobility of the ice cover in the LIA. Together with the evidence from around the Arctic Ocean and from areas of perennial (landfast) sea ice in proximity to the Lincoln Sea (lines 157-187), however, the new datasets are in favor of a transition to a predominantly seasonal sea ice regime in the Arctic Ocean during the Early Holocene.

3. Also clarify the point above (relationship with central Arctic Ocean sea ice conditions) in the abstract and first paragraph of the text.

   We have added an equivalent statement to the abstract (lines 19-20).

4. Line 92/Fig 2: The mention of an ostracod species that likely indicates open water conditions during the early Holocene inferred "low ice period" is a great complement to the biomarker data, and significantly strengthens the key conclusion of this paper. But the ostracode angle seems just sort of "thrown in" to the paper and I want more info. How well does occurrence of Polycope align with the biomarker evidence, for example? It appears the data are presented in a separate 2022 paper, but can you show the published ostracod species (assemblage?) data here in Fig 2? At least, show occurrence vs absence of the key ostracod taxon Polycope.

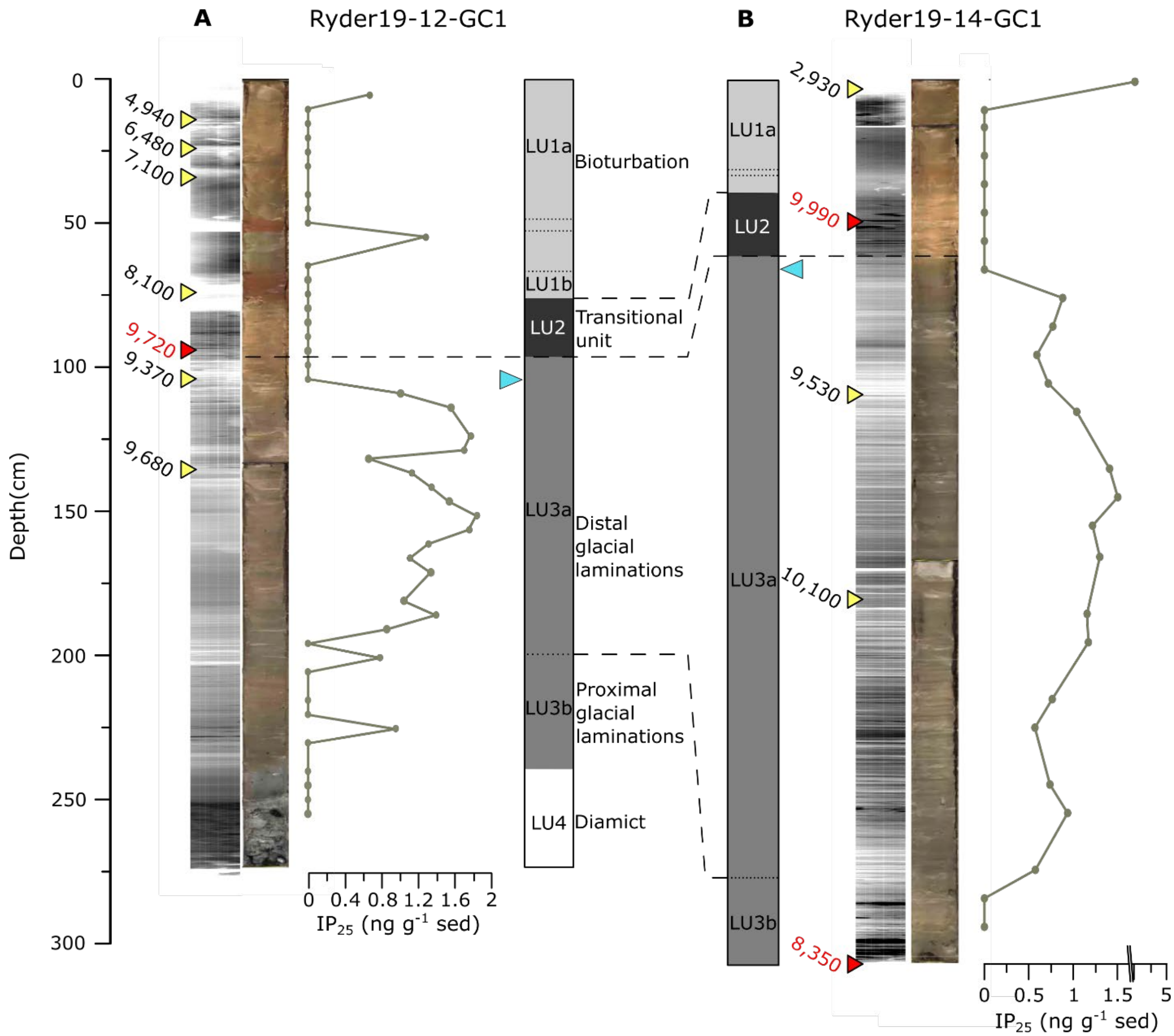
   Thank you very much for this comment. We have revised and expanded the comparison with the micropaleontology data (115-140). We have also included supplementary figures showing the abundance of the benthic foraminiferal species *Cassidulina neoteretis* and *Oridorsalis umbonatus* and the ostracod species *Polycope* spp. and *A. arcticum* (for 14GC only) (Fig. S6, Fig. S7). *C. neoteretis* is the dominant benthic foraminiferal species, typically associated with bottom waters of Atlantic origin, while *O. umbonatus* is characteristic of environments with low export of organic carbon to the seafloor, such as under the perennial pack ice of the central Arctic Ocean. *A. arcticum* lives parasitically on the under-ice amphipod *Gammarus*, which is found exclusively in the brackish surface layer underneath pack ice. Thus, *A. arcticum* can be used as an indicator of the perennial sea ice cover of the central Arctic Ocean and does not occur on continental shelves characterized by seasonally ice-free conditions. Although *Polycope* spp. is typically described as a genus associated with seasonal sea ice environments/marginal sea ice, based on its association with high productivity, it also occurs in surface sediments of the central Arctic Ocean under perennial sea ice. It has previously been described as inhabiting the upper Arctic Ocean Deep Water and/or lower AIW Arctic Intermediate Water influenced by waters of Atlantic origin. We have thus revised our discussion of the micropaleontological data accordingly.

5. I'd like to see acknowledgement and a brief discussion of the "binary" nature of the signal here in biomarkers. In particular, is it surprising that the early Holocene period of seasonal sea ice would end so abruptly in an (essentially permanent, except for one "blip" in the mid Holocene and the recent change) transition to perennial sea ice? The sharpness of that change in biomarker abundance makes me wonder if something other than a climate shift is responsible, ie if something drove a fundamental change in overall sedmentation processes at the site at that time. But did any other basic sedimentological parameters change at the same time? It's hard to tell when comparing Supp Figs with Fig 2 by eyeballing.

   Thank you for this comment. It is true that the signal of IP_{25} is 'binary' (i.e. present during the early Holocene and absent during the mid-to-late Holocene) with a few exceptions. This is in part due to the methodological approach that we have chosen. As mentioned in the method section (lines 725-728) concentrations of highly branched isoprenoids are very low, which did not allow us to derive a full mass spectrum (in SCAN mode) of IP_{25} for the Ryder19 samples. Identification of the isoprenoid peaks is thus based on the retention time compared to a well quantified reference sediment and SIM mass spectra. Typically, HBI II and IP_{25} co-vary in the Arctic, where HBI II usually has higher concentrations than IP_{25}. Thus, for additional quality control, we only quantified IP_{25} in samples where we could also identify a distinct HBI II peak (lines 725-728). While the drop in IP_{25} around 9.7 cal ka BP is a true feature, IP_{25} concentrations may not have been zero throughout the entire mid-to-late Holocene. Instead, they are below the level of quantification. This is in line with the trends in TOC and sterols, which show low (but not zero) concentrations during the mid-to-late Holocene. True zero values would also be unrealistic since there is always some degree of transportation of these compounds to the site.

Similarly sharp decreases in IP_{25} concentration/fluxes are also observed in records from the northernmost Barents Sea and the inner NE Greenland Shelf (Fig. S8) at 9.1 cal ka BP and 9.6 cal ka BP, respectively. Today these regions are characterized by perennial sea ice, similar to the Lincoln Sea.

The highest concentrations of IP_{25} in 12-GC and 14-GC are associated with the lithological unit (LU) 3a, characterized as distal glacial laminations (Review figure 1). These are a product of meltwater plumes derived from the grounding zones of nearby glaciers, such as Ryder Glacier, which terminated in an extensive ice tongue at the time (M. O'Regan et al., 2021). The decrease in IP_{25} occurs near (but prior to!) the transition from LU3 to LU2 in both cores (Review Figure 1). Thus, the decrease in IP_{25} is not directly linked to a lithological boundary in the cores, suggesting a climatic rather than a lithological reason for the drop in IP_{25} concentrations.



Review Figure 1. Comparison of IP_{25} concentrations in (A) Ryder19-12-GC1 and (B) Ryder19-14-GC1 with lithological observations. For each core the radiograph, core image, IP_{25} concentrations and lithological interpretations are shown. The blue triangles represent the depth of IP_{25} decrease.

Nonetheless, we are considering potential lithological reasons for the decrease in IP_{25} below:

The change from LU3 to LU2 is associated with a drop in sedimentation rate (Fig. S3) from 67 cm/ka to 15 cm/ka and an increase in the energy of the depositional environment (slightly coarser material, mottled/lenticular sedimentation). Changes in the depositional environment can cause changes in the preservation of organic carbon and with it biomarkers such as IP_{25}. The key drivers of sedimentary TOC concentrations are the input (primary productivity and water depth), the preservation, and the dilution. At sedimentation rates >35 cm/ka the preservation of autochthonous marine organic matter is largely independent of the oxygenation of bottom waters and instead depends on input and dilution alone (Tyson, 2001). Assuming an oxic environment, a decrease in sedimentation rates from 67 cm/ka to 15 cm/ka would be associated with an increase in the sensitivity of TOC concentrations to the oxygenation of bottom waters via the burial efficiency. At the same time, however, the dilution of TOC by inorganic particles decreases, which in sum should cause a slight increase in TOC burial if the input remains constant (Tyson, 2001) (Review Figure 2).

Review Figure 2. Fig. 2 from Tyson (2001): Relationship between TOC and sedimentation rates assuming 800 m water depth and a primary productivity fixed at 100 gC/m^2/a (thus a constant DF) plus a range of carbon burial efficiencies from 10 to 75%: (a) TOC axis linearly scaled, (b) TOC axis log scaled [symbols as in (a)]. Note the rapid divergence in (SR-controlled) TOC values, versus anoxic TOC values (50–75% BE).

The change in sedimentation rate is thus unlikely to be the driver of the decrease in TOC and IP_{25} concentrations prior to the LU3/LU2 boundary. Alternatively, an increase in particle size associated with a decrease in surface area may affect TOC concentrations, as organic carbon is adsorbed to mineral surfaces (Adams and Bustin, 2001). We do not have particle size data, but the core log describes a gradual transition from clayey-silt to silty-clay in the upper LU3a of 12-GC, which might contribute to a decrease in the burial efficiency of organic carbon.

Alternatively, glacial dynamics, which affected the lithology in the Lincoln Sea, may have affected sea ice conditions at the same time. Under modern conditions, it has been shown that enhanced meltwater discharge at the grounding zone of neighboring Petermann Glacier, which ends in an extensive ice tongue, increases the upwelling of relatively warm recirculated Atlantic Water (supplementary materials lines 32-51). During the Holocene, benthic foraminiferal assemblages in the southern Lincoln Sea and Sherard Osborn Fjord suggest continuous presence of bottom waters of Atlantic origin (lines 115-140; Fig. S6, Fig. S7). Thus, enhanced meltwater discharge at the grounding zone of Ryder Glacier may have increased the incorporation of relatively warm waters of Atlantic origin into the surface mixed layer, promoting a decrease in the sea ice concentration. At around 9.3 ± 0.4 cal ka BP the grounding zone of Ryder glacier retreated behind the inner sill in Sherard Osborn Fjord (Matt O'Regan et al., 2021), which under present day conditions blocks the progression of bottom waters into the fjord (Jakobsson et al., 2020). A reduction in upwelling of Atlantic-derived waters may thus have contributed to the re-establishment of a perennial ice cover in the southern Lincoln Sea.

Thus, while we cannot rule out that changes in the particle size and upwelling of water of Atlantic origin contributed to the observed sea ice signal, the decrease in biomarker concentrations prior to the lithological boundary suggests that climatic drivers, such as atmospheric temperatures and dynamic changes related to the whole Arctic Ocean sea ice regime played the dominant role.

6. Lines 121-130: Two issues I'd like to see discussed here. One, what role is diagenesis likely to play in composition of these surface sediments vs deeper sediments? And two, is it likely that the last century dominates signals in these surface sediments given the stated durations they likely represent? Wasn't the past 1 ka until the 20th Century especially cold in the Arctic, and if so, why are values so high? Overall, what I'm getting at is: How comparable are these surface values to downcore values (given diagenesis) and what time period do they likely represent?

   Thank you very much for your comment. For the purpose of this study, we assume that the degradation/diagenesis of biomarkers is positively correlated with that of the total organic carbon (TOC). We have added this information to the supplementary material (lines 3-10). This suggests that the biomarker values normalized to TOC represent a 'degradation/diagenesis-corrected' signal. Since the biomarker concentrations (both downcore and in the surface sediments) normalized to sediment mass and TOC are positively correlated (Fig. S4), we assume that degradation does not have a significant effect on the overall trend of the biomarker record. For completeness, we have added the biomarker concentrations in the surface sediments normalized to TOC to Figure S4.

   Based on sedimentation rates of 1.4-3 cm/ka in the top of the gravity cores, the multi-corer surface samples (top 2 cm in 12GC, top 1 cm 13GC) likely cover the last 650-750 years, but are heavily influenced by recent decades because of the higher biomarker concentrations. We have added this information to the supplementary material of the manuscript (lines 110-116). Taking the downcore record into account, which shows that periods of extensive sea ice are marked by extremely low biomarker concentrations in the southern Lincoln Sea, it is very likely that the surface samples are biased towards recent times (the last century/decades) when sea ice concentrations in the southern Lincoln Sea decreased. The sediment accumulation rate in the upper few mms is also typically much higher in marine sediment cores and this further contributes to the bias to modern values.

7. Lines 132-135: In addition to the Agassiz ice core data, there are additional archives and proxies that speak to early Holocene atmospheric temperatures and sea ice conditions in this region that should also be part of this paper's discussion. This would strengthen the paper's case for the link between summer air temperatures in the region and sea ice conditions in the Lincoln Sea, and avoid the appearance of cherry-picking one record for comparison. Even if Agassiz is the only proxy record with which direct quantitative comparisons are made, the broader literature should be discussed and acknowledged. This study should be grounded in the broader

literature from the region. Axford et al 2021 Annual Review of Earth and Planetary Sciences reviews temperature evidence from Greenland. Or as examples of individual studies, this paper could make interesting links to the ice cap histories reconstructed in North Greenland by Larsen et al 2019 Boreas (which proposes that reduced early Holocene sea ice and associated higher precip may have helped some ice caps in northern Greenland survive the thermal maximum), and summarize more sea ice reconstructions eg from Georgiadis et al 2020 Marine Geology on Holocene sea ice in Nares Strait.

Thank you very much for this comment. In the revised version of the manuscript we provide a more detailed introduction to Holocene temperature records from around Greenland (lines 192-211) as well as more detailed comparisons to sea ice records from around the Arctic Ocean and in proximity to the Lincoln Sea (lines 159-179). We did not include Georgiadis et al. (2020), as the record first starts at 9 ka. Additionally, Nares Strait was still closed during the Early Holocene, likely resulting in different sea ice conditions at its northern and southern end.

8. Lines 206-218: I really appreciate the effort to link a paleoclimate record directly to implications for the future. However, there's a problem with this argument, in its assumptions about seasonality. Above you argued for the link between summer air temperatures and sea ice conditions – but here you discuss mean annual air temperatures. You cannot assume that the modern relationship between JJA and MAAT held up in the early Holocene. Indeed, seasonality was most likely very different in the early Holocene given insolation forcing. Reconsider your rationale here, with careful attention to non-stationary seasonality through the Holocene.

   Thank you very much for this comment. We acknowledge the reviewer's comment that we did not take non-stationary seasonality into account in the previous manuscript draft. This was due to the fact that both the summer temperatures and mean annual air temperatures (MAT) at Agassiz ice cap and in the southern Lincoln Sea are correlated with modern sea ice dynamics (Fig. S9). However, we chose to only show summer temperatures as these are directly comparable to the Early Holocene temperatures reconstructed from melt layers in the Agassiz ice cores, while the reconstructed mean annual temperatures are shown as the anomaly from pre-industrial (MAAT). Thus, reconstructed MAAT and MAT from reanalysis data are not directly comparable. In the revised manuscript draft we show that sea ice correlates with both summer temperatures and MAT in the southern Lincoln Sea during the last ~40 years (Fig. S9). Thus, our argument on future annual air temperatures and sea ice dynamics in the southern Lincoln Sea (lines 286-302) holds true.

9. Additional comments:

   Abstract line 21: As mentioned above, it's not only Agassiz data that show warm summer air temperatures in this region in the early Holocene. Broaden scope.

   Thank you very much for this comment. As listed above, we have now included more records for this comparison, so in the interest of word count, we have deleted the reference to the Agassiz ice core from the abstract. The sentence now simply states a close correlation of sea ice conditions in the Lincoln Sea with atmospheric temperatures, but does not refer to a specific temperature record.

Abstract line 22: suggest replacing “amplified temperatures” with “amplified warming” or “amplified temperature change”

We have changed the wording to ‘amplified warming’.

Fig 1 caption: Cool figure. For the sake of your broad readership, clarify at this first use what is meant by annual sea ice concentration trend in % per decade. % of what exactly?

Sea ice concentration (SIC) is typically reported in %, with 100% suggesting perennial sea ice at a given location. The SIC trend in % per decade thus shows the decrease/increase in sea ice concentration averaged over 10-years. We have added more explanatory information to the figure caption.

Line 52: so long-term stability/prognosis is unknown. Set this up by summarizing the range of reasonable climate model projections/results. Do models disagree or is there a consensus projection? Current wording here implies no consensus ie model disagreement, but describe the state of knowledge more precisely.

Thank you very much for this comment. We have rephrased the wording in the introduction based on your second major comment’ regarding the relationship of sea ice cover in the LIA and that in the central Arctic Ocean (lines 55-59). The introduction now clearly states that there is model consensus on the LIA being the last region of summer sea ice in the Arctic in a warming climate.

Line 60: See my previous requests regarding presentation of age modeling.

We have revised the precision of the ages given in the manuscript text to reflect the uncertainty regarding ^{14}C dating in the Arctic Ocean. However, we are not quite sure what the reviewer refers to in the introduction, where we are simply stating that the age model was created using lithological investigations and ^{14}C dating. To us this is important information, crucial for the readers.

64: Reference 19 is not a great one to make this point here, since it has very limited Arctic data. Choose a reference that includes (or even emphasizes) Arctic data eg Kaufman’s 2004 Arctic HTM synthesis in QSR, or Kaufman et al 2020 Scientific Data for a global perspective that includes the Arctic.

Thank you very much for your comment. We have replaced the reference with Kaufman et al. 2020 (Scientific Data).

Fig 2: What do the authors make of the increase in IP25 in the late Holocene in both cores (especially notable in core -14)?

Thank you very much for your comment. Regarding the low sedimentation rates in the top of both cores, we assume that the increase in IP25 may be related to the recent decrease in sea ice in the Lincoln Sea region and effects of bioturbation, especially pronounced when sedimentation rates are low (lines 105-114).

143-144: By “numerical studies” do you mean climate models? This seems unconventional terminology. And more importantly, is this a consensus view or do models vary? Either way is interesting – just clarify.

We have changed numerical studies to climate model simulations throughout the entire manuscript. Not many modelling studies on sea ice during the earliest Holocene exist. We focus on two studies: Dyck et al. (2010) and Stranne et al (2014). While Stranne et al. 2014 show a transition to seasonal sea ice in the entire Arctic Ocean associated with early Holocene radiative forcing, Dyck et al. 2010 suggest that the sea ice concentration predominantly diminished in the marginal seas, while sea ice thickness decreased everywhere in the Arctic Ocean. We have added this information to the manuscript (lines 212-218).

150-152: This point abou the Lomonsov Ridge data is important, but hard to make sense of as the paper is currently written. More clearly articulating the relationshpi between future & past ice conditions in the central AO vs the LIA (earlier in the paper, per my earlier comment) will hopefully help.

Thank you very much for your comment. In relation to your major comment 2, we have clearly stated the proposed relationship between LIA and Arctic Ocean sea ice dynamics based on climate models (lines 55-59). Climate models consent that the LIA will be the last remaining area of summer sea ice in the Arctic Ocean under future anthropogenic warming, indicating that a transition to seasonal sea ice in the LIA is equivalent to a transition in the entire Arctic Ocean. However, as the reviewer points out, records from the central Arctic Ocean disagree with a transition to seasonal sea ice during the early Holocene. While sedimentation rates are low and only two data points cover the interval of the early Holocene, we cannot rule out the possibility that a thinner but (near) perennial sea ice cover remained in the central Lincoln Sea and adjacent central Arctic Ocean. In this case the two core sites in the southern Lincoln Sea likely record a signal of thinner more mobile ice, associated with the break-up of the landfast ice cover (lines 178-187).

154: This is a key conclusion of your paper, but it needs to be clarified with respect to conditions in the central AO, per my earlier comments.

We have provided a more detailed discussion on this point adding information on additional biomarker records from across the Arctic Ocean (lines 157-188).

159: again, “numerical investigations” = numerical climate model simuations?

This has been changed.

182-183: Maybe I am missing something obvious, but I don’t understand the temperature values given here. Are these meant to be anomalies relative to a baseline, and if so, why eg is HTM 5 degrees cooler (?) than pre-ind? I think there’s something in the wording here that is confusing. Clarify or correct.

These are the average temperatures for the interval of seasonal sea ice during the Early Holocene as reconstructed from Agassiz ice cap. TJJA was reconstructed based on the meltlayer record and these are absolute temperatures, while the mean annual air surface temperature anomaly is the anomaly from pre-industrial. We have restructured/reworded the sentence slightly to avoid confusion.

Fig 3 caption (or elsewhere): clarify what the Agassiz temperature inferences are based upon (ie what are the proxies – melt and d18O)

Thank you for this comment. We have added the information to the figure caption of Fig. 2 (lines142-158).

198: decreased (not decrease – fix tense)

This has been changed, thank you very much.

Supp Info: Some of the main points in the "Additional climate mechanisms" section of Supp Info should, in my opinion, be at least summarized briefly in the main text of the paper, to provide a more nuanced discussion within the paper itself. There is a nice, rich discussion within Supp Info that is helpful and paints a necessary, much more complete picture of possible influences on early Holocene sea ice in the LIA/Lincoln Sea. Authors could integrate key aspects of it into the main text to significantly strengthen the discussion.

Thank you very much for this comment. To keep the discussion as focused as possible, we have refrained from including a long discussion of the potential additional mechanisms, especially with regard to the freshwater/AW incorporation mechanisms, as this is highly speculative. However, we have included a sentence stating other potential mechanisms with a reference to the supplementary text (lines 226-229).

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27th Jan 23

Dear Dr Detlef,

Please allow us to apologise for the delay in sending a decision on your manuscript titled "Seasonal sea ice in the Arctic's last ice area during the Early Holocene". It has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. In particular please place the study in the appropriate context for future implications by explaining that this is a semi-quantitative test and specify in more details in your implication section the role of the air temperature as the main driver for your results. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The study by Detlef et al. addresses a pressing need for natural baseline values of sea ice variation in the Arctic. Sea ice fluctuations in the Lincoln Sea have been reconstructed using marine sediment cores and paleo proxies of sea ice and ocean temperature. The study finds a transition from perennial sea ice to seasonal sea ice during early Holocene a period with conditions and air temperatures warmer than pre industrial levels. The study imply that future increasing air temperatures will cause sea ice in the Lincoln sea to change from perennial to seasonal sea ice. All these findings are of high interest to a wider field both of scientists and policy makers. The last section on implications could be strengthened making it clear that this is a semi-quantitative test, where the only the main driver, air temperature, is considered.

Reviewer #2 (Remarks to the Author):

The authors have done an excellent job at responding to the reviewers' comments. I recommend publication.

Reviewer #3 (Remarks to the Author):

The authors have provided detailed responses to my comments, and where needed have revised their manuscript to clarify or strengthen points. I appreciate their thorough response to reviews, and enthusiastically recommend the revised paper for publication.

Rebuttal letter COMMSENV-22-0811A

Reviewer #1

The study by Detlef et al. addresses a pressing need for natural baseline values of sea ice variation in the Arctic. Sea ice fluctuations in the Lincoln Sea have been reconstructed using marine sediment cores and paleo proxies of sea ice and ocean temperature. The study finds a transition from perennial sea ice to seasonal sea ice during early Holocene a period with conditions and air temperatures warmer than pre industrial levels. The study imply that future increasing air temperatures will cause sea ice in the Lincoln sea to change from perennial to seasonal sea ice. All these findings are of high interest to a wider field both of scientists and policy makers. The last section on implications could be strengthened making it clear that this is a semi-quantitative test, where the only the main driver, air temperature, is considered.

Thank you very much for recognizing the significance of our work. We agree that a statement regarding which drivers are considered in the 'Implications' section was missing. We have added the following paragraph to the beginning of the section:

In the following section, we are discussing the implications of the early Holocene sea-ice conditions in the Lincoln Sea for the future stability of sea-ice in the LIA. Given the correlation of sea-ice and atmospheric temperatures during the early Holocene, we are focusing on atmospheric temperatures as the main driver of sea-ice change, not considering additional drivers such as changes in the Arctic Ocean surface circulation, effects of freshwater addition to the Arctic Ocean, and local sea-ice dynamics, including the formation of ice arches in Nares Strait.

Reviewer #2

The authors have done an excellent job at responding to the reviewers' comments. I recommend publication.

Reviewer #3

The authors have provided detailed responses to my comments, and where needed have revised their manuscript to clarify or strengthen points. I appreciate their thorough response to reviews, and enthusiastically recommend the revised paper for publication.

We thank both reviewer #2 and #3 for recommending our work for publication in *Communications Earth and Environment.*